

On the Orbit of the First Comet of 1850.

By R. C. Carrington, Esq.

The readers of the *Astronomische Nachrichten* will recollect that in No. 803 Mr. Sonntag, assistant at the Altona Observatory, published an orbit of Dr. Petersen's third Comet, which he had very elaborately deduced from a careful discussion of all the European and American Observations, about 400 in number. Mr. Sonntag's conclusion was, that the orbit was sensibly parabolic, the numerical deviation resulting from his equations of condition being many times less than its probable error. It was hardly to be expected, after this, that further observation and discussion would materially modify his result, yet such I have found to be the case, and the result is instructive; for though in ordinary cases it would excite no surprise that an excentricity should be elicited by an increase of the arc of observation, Mr. Sonntag's result appeared too exact and too well based to countenance the expectation.

The comet's excentricity was not, however, its only feature of interest. At the bottom of page 155 of vol. x. of the *Monthly Notices*, will be seen the paragraph, "Mr. Gauss is even of opinion that it is not *proved* that a comet must, after having passed the perihelion, move in the same orbit as before." I cannot say that I apprehend the exact idea contained in this sentence, although I can read it in more ways than one; yet I think my calculation will afford a satisfactory answer to it. The orbit of this comet after perihelion fits perfectly to that before. This, although not a general proof, is a particular instance.

In residual phenomena, attributable to physical causes extraneous to gravitation, this comet offers "no case for a jury," the final uncompensated differences of the normals being of a magnitude attributable to observation.

My calculation is, strictly speaking, a combination of Mr. Sonntag's. I adopted at once his eight normal positions, not thinking a recomputation of the European observations would repay the time it would take: to those I was enabled to add two other normals, drawn from the very excellent observations made by Mr. Maclear himself at the Cape of Good Hope, Mr. Sheepshanks having kindly allowed me to take a private copy. For the whole period of visibility the perturbations of the six innermost planets were calculated and applied according to the new co-ordinate method of Professor Encke. In this part of the work I derived assistance, which I am happy here to acknowledge, from the annotated translation published by the Astronomer Royal in the *Nautical Almanac* for 1856. In this paper I pass at once to my result, as I have sent a fuller account to Dr. Petersen for insertion in the *Nachrichten*.

The elements represent the instantaneous orbit of May 12, 1850 (at Greenwich, noon):

T		1850, July 23 ^h 56 ^m 51 ^s 69	Berlin Mean Time.
π -N		18 ^o 31' 36".74	} Mean Eq ^x . 1850 ^o 0.
N		92 53 27.84	
I		68 11 23.56	
e		0.9988519	
q		1.0814488	Motion direct.

leaving the following uncompensated normal differences :—

	$\Delta \alpha \cdot \cos \delta$	$\Delta \delta$	
1. May 10 ^o	+5.88	-1.88	
2. 21 ^o	-3.05	-0.86	
3. 30 ^o 5	+0.01	-0.93	
4. June 7 ^o 5	-0.57	+0.67	
5. 21 ^o	-0.19	+1.67	
6. July 2 ^o 5	-0.07	-0.70	
7. 12 ^o	+0.47	-0.54	
8. 21 ^o	-1.06	+1.01	
9. Sept. 15 ^o	+1.28	-0.48	} The Cape Normals.
10. Oct. 5 ^o	-0.36	-0.65	

The period corresponding to the excentricity given above being somewhat short of 29,000 years.

Letter from M. Yvon Villarceau to the Astronomer Royal.

“ I have the honour to transmit to you along with this letter a copy of a Memoir which I have just presented to the Academy of Sciences, upon the double star *Coronæ*. I shall be gratified if you should find it worthy of being presented to the Astronomical Society.*

“ Allow me to avail myself of this circumstance to give you some explanations upon a subject of controversy, which appears to have excited the attention of the Astronomical Society.

“ In a memoir upon the comet of D’Arrest, I expressed the opinion that the observations of that body, made with the heliometer of Königsberg on the occasion of its last apparition, might be more precise than those which have been obtained elsewhere during the same period. M. Otto Struve appears to conclude from this remark, that, in general, I prefer heliometer observations of comets to those made with a refractor as powerful as that of Cambridge, and he declares that he does not concur with me in adopting this preference. The circumstance which induces him to reject the heliometer in its application to observations of comets consists in

* An abstract of M. Villarceau’s Memoir will be found at p. 229.